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DO FIRMS LEARN TO CREATE VALUE? THE CASE OF ALLIANCES

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We investigate whether firms learn to manage interfirm alliances as experience accumulates. We use contract-specific experience measures in a data set of over 2000 joint ventures and licensing agreements, and value creation measures derived from the abnormal stock returns surrounding alliance announcements. Learning effects are identified from the effects of unobserved heterogeneity in alliance capabilities. We find evidence of large learning effects in managing joint ventures, but no such evidence for licensing contracts. The effects of learning on value creation are strongest for research joint ventures, and weakest for marketing joint ventures. These results are consistent with the view that learning effects are more important in situations characterized by greater contractual ambiguity.

INTRODUCTION

Alliances create value (Chan *et al.*, 1997; McConnell and Nantel, 1985).[1] Yet, there is widespread recognition of the difficulty inherent in this process of value creation, as evidenced by the large fraction of firms that fail to do so, by the numerous academic publications highlighting the failure of alliances (see, for example, Kogut, 1989), and by the wisdom among practitioners.[2]

What, then, drives value creation in alliances? Our empirical analysis points to two important factors: a firm's experience in managing alliances, and the existence of persistent firm-specific differences in the ability (or inability) to create value through alliances.

Alliances are complex organizational forms that are usefully viewed as incomplete contracts.[3] They typically involve the transfer of know-how between firms, a process that is fraught with ambiguity (Jensen and Meckling, 1991). Like other complex organizational forms, it is difficult to prespecify the contingencies that arise in their management. For example, unanticipated changes in the environment may alter the incentives of the contracting parties; intangible personal, organizational, and cultural attributes may affect the ongoing relationship between firms in

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[1]Alliances are organizational forms that allow otherwise independent firms to share resources of a variety of sorts. Conceptually, we think of them as intermediate organizational forms between markets and hierarchies.

[2]Thus, the CEO of Emerson Electric refers to implementation as 'the graveyard of strategic alliances.' Another top alliance manager claims that managing alliances is as difficult as 'stirring concrete with eyelashes.' A 1997 survey on 'Institutionalizing Alliance Capability' by a prominent consulting firm reported wide discrepancy in failure rates among those firms generally thought to be able to manage alliances well, and among those thought to be poor at managing alliances. (*Alliance Analyst*, December 23, 1996; June 9, 1997; August 15, 1997).

[3]The trade press sometimes refers to alliances in this fashion, e.g., 'alliances are incomplete contracts (which) leave all sorts of room for maneuver and interpretation' (*Alliance Analyst*, November 25, 1996).

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important ways as well. Clearly, acquiring and assimilating the information needed in order to specify and react to all such contingencies is costly (Simon, 1955). Consequently, there may be important learning dynamics in a firm's ability to anticipate some of these contingencies, or in its ability to respond to them in an effective manner. In addition, since the management of alliances is not a well-defined process, there are likely to exist differences across firms in their ability to manage these; indeed, if the ambiguities involved with managing alliances were perfectly specifiable, it is unlikely that interfirm differences in the ability to create value through alliances would persist.[4] Thus, incomplete contract theory suggests that both learning effects and unobserved heterogeneity might be important determinants of value creation through alliances.

While previous studies have examined the consequences of learning in alliances, and implicitly pointed to the importance of interfirm heterogeneity in managing alliances, neither of these issues, surprisingly, has received much attention empirically. This paper is an attempt at answering many outstanding questions in this area, specifically: (1) Do firms learn to create value via alliances; and, how important are these learning effects? Consequently, can alliance capabilities be acquired or developed by firms? (2) When is learning important? Indeed, which kinds of alliances are most susceptible to the kinds of behavior that have been highlighted in the literature as resulting from learning dynamics? (3) Are interfirm differences in 'alliance capabilities' empirically important? If so, are these capabilities general-purpose or alliance-specific?

Joint ventures and licensing arrangements offer a particularly useful venue within which to examine these questions for several reasons. First, alliances—of which joint ventures and licenses are the two most common examples—have become one of the most important organizational forms to emerge in the past decade, with more than 20,000 such reported alliances in just the last 2 years worldwide. They thus constitute an intrinsically interesting organizational form. Second, many large firms have entered into several dozen alliances, and occasionally hundreds of them. Consequently, many of these firms have built up substantial experience bases. From an empirical standpoint, alliances therefore offer an ideal arena within which to study the management of organizational forms because of the potentially large variation across firms in their value creation through alliances, and because firms differ widely in terms of both their experience with alliances and, according to practitioners, their skill in managing alliances. Because of these sources of variation, alliances also offer a better arena to examine the effects of learning than the relatively less frequent event of an acquisition. Finally, considerably more information is available on firms' alliance activities than on other activities internal to the firm, even though the latter might constitute important sources of firm learning as well.

We find strong learning effects in joint ventures, though none in licensing contracts. Within joint ventures, the learning effects are especially strong for research joint ventures and production joint ventures, and weak for marketing joint ventures. It may be that those firms with more experience in managing alliances also differ in other (unobserved) ways from other firms, thus confounding the effects of learning and unobserved heterogeneity. Since we have multiple observations on each firm, we can effectively resolve this identification problem as well. Indeed, we find strong evidence of firm-specific alliance capabilities in all subsamples. The learning effects, however, are robust to allowing for such unobserved differences in capabilities across firms.

In the next section, we review related literature. Subsequent sections sequentially present our data, the estimation methodology, the results and robustness checks, and a concluding discussion.

THEORY AND HYPOTHESES

In this section, we place this study in the context of existing theoretical and empirical studies on learning by firms, relate it to the existing literature on firm heterogeneity, and develop testable hypotheses.

Learning to manage organizations

The notion of learning can be equated with improvements in the ability to anticipate and

[4]Authors have long opined that such intangible capabilities constitute the heart of what is distinctive about a firm. Witness Blau and Scott's (1962) emphasis on the unwritten rules within an organization, and Barnard's (1938) suggestion that such intangible capabilities cannot be known unless one works within the organization.

respond to contingencies that cannot be prespecified in a formal contract. If all contingencies could be prespecified perfectly, then responses to these contingencies could also be prespecified, and there would be little scope for learning. Academics have used various terms to describe such processes of anticipating and responding to contingencies. For example, Argyris and Schon (1978) developed models of organizational learning like 'error detection and correction in theories-in-use.' Firms are said to possess 'routines' and 'capabilities' when they have learned to perform some function with sufficient distinction relative to some comparison group (Cyert and March, 1963; Nelson and Winter, 1982). Such knowledge is often referred to as 'tacit' (as opposed to 'codified'), with the implication that such knowledge is inaccessible to other firms, absent their own learning.

A large managerial literature similarly discusses the importance of learning to manage organizational forms. Volumes have been written, for example, on the nuances of managing acquisitions (Haspeslagh and Jemison, 1991; Singh and Zollo, 1999), or on learning to manage cross-border entry (Chang, 1995). Examples of specific firms excelling at such learning also abound. Hansen Trust has learned to manage acquisitions, Thermoelectron to manage spin-offs (Allen, 1998; Baldwin and Forsythe, 1982); while Xerox was often cited as a firm that had failed to do so (Smith and Alexander, 1988).

Given the importance of learning in both anecdotal accounts and formal theories, it is surprising that no systematic empirical evidence exists to indicate either how important the role of learning might be,[5] or *when* it is likely to be important. Answering the question of how easy or difficult it is for firms to acquire capabilities over time should also shed some light on the normative implications of theories that study the role of interfirm differences in such capabilities or resources. This literature suggests that the capability to manage a complex organization is tacit, costly to develop, and hard to imitate. Even here, however, large-sample evidence for cross-sectional variation in such capabilities is difficult to come by.[6] We turn to the specific context of alliances next, which is the focus of this study.

Learning to manage alliances

An alliance can be viewed as an incomplete contract between firms, in the sense that detailed interactions between the alliance partners can rarely be fully prespecified. Therefore, the theoretical literature reviewed above would, by extension, suggest that alliances are likely to be difficult to manage. One reason for such difficulties might revolve around the complexities surrounding interfirm knowledge transfers, an important part of many alliances. Several authors have discussed the difficulties of transferring tacit know-how (Winter, 1988; Jensen and Meckling, 1991; Szulanski, 1996), and others have emphasized that such difficulties are likely to be more pronounced in an interfirm setting than in an intrafirm setting (Baker, Gibbons, and Murphy, 1997: 29). Such transfers of knowledge or information are at the heart of related studies that focus on the process by which firms learn from a particular alliance. These studies acknowledge the tension between competition and cooperation within alliances (Hamel, 1991; Gulati, Khanna, and Nohria, 1994; Khanna, Gulati, and Nohria, 1998; Khanna, 1998; Anand and Galetovic, 1999). Building on this literature, Kale, Singh and Perlmutter (2000) empirically examine the role of relational capital between alliance partners as a means of both enhancing cooperative behavior and mitigating competitive conflicts.[7] Recent work has suggested that such relational capital, which allows a firm to learn from its alliance partner, is a function of characteristics

[5]Curiously, such empirical evidence as does exist has to do primarily with the celebrated 'learning curve' literature (Spence, 1981; Fudenberg and Tirole, 1984; Lieberman, 1984; Ghemawat and Spence, 1985), which focuses on learning to reduce production costs. We conjecture that the reason for this is that production costs have historically provided the most convenient data to examine learning effects.

[6]Theoretical foundations for the persistence of firm heterogeneity date back to Selznick (1957) and Penrose (1959). Several authors have found that firm effects account for a lot of the variation in profit rates across firms (Cool and Schendel, 1988; Rumelt, 1991; McGahan and Porter, 1997), while others have found strong firm effects in managing research (Henderson and Cockburn, 1994).

[7]In a related case study, Dyer and Nobeoka (2000) examine how information flows between Toyota and its suppliers both enhances the latter's incentives for specific investments and mitigates free-riding via implicit contracts. Interestingly, Afuah (2000) provides evidence on the *costs* of strong relationships. He shows how strong relationships may lock-in the firm to existing technologies, thereby disadvantaging the firm in periods of drastic technological change.

of the dyad in question, rather than of either of the individual firms (Lane and Lubatkin, 1998). Indeed, the capability to learn may be partner-specific (Dyer and Singh, 1998). A related stream of work has begun to develop useful taxonomies of alliance learning strategies (e.g., Larsson *et al.*, 1998). These efforts have been aided by discussions of detailed case studies on how learning unfolds in alliances (Doz, 1996; Arino and de la Torre, 1998). It is important to note that all these papers have focused on the process of learning *within* a particular alliance. In contrast, our analysis is primarily concerned with whether firms exhibit learning effects *across* a portfolio of alliances. Effectively, therefore, our focus is on the question of *learning to learn* from alliances.

How exactly might firms learn to manage alliances, or acquire an alliance capability? Much theoretical work on learning is relevant to this question. The question of how firms learn can be broken down into, first, how individuals within the firm learn and, second, into how firms harness the learning experiences of such individuals. Following the related discussion in Cohen and Levinthal (1990), these two aspects can be considered in order.

Repeated exposure to sequences of alliance partners exposes individuals within the firm to a broad repertoire of experiences. This facilitates the interpretation of new unforeseen contingencies in their subsequent alliance interactions. Bower and Hilgard (1981) suggest that it is easier for an individual to learn from new experiences, the greater the number of stored objects and instances in her memory. Indeed, the ability to learn from a particular alliance is likely to be enhanced by the trials and tribulations of past learning experiences. Some authors have observed that the knowledge being built up in this way may be about learning skills themselves (Ellis, 1965), a phenomenon which Estes (1970) refers to as 'learning to learn'.

Cohen and Levinthal (1990) develop the idea that 'learning to learn' at the firm level is a complex function of the individual-level phenomenon. It depends on how the firm communicates with sources of knowledge outside the firm, on the mechanisms within the firm that exploit individual experiences, and on the distribution of expertise within the firm. There may not be a unique, optimal mechanism that allows firms to learn from these experiences. For example, heterogeneity in individuals' knowledge and experiences will make it difficult to disseminate newly acquired knowledge within the firm, but will generally facilitate the absorption of knowledge from *outside* the firm, thus creating trade-offs.[8] Cohen and Levinthal (1990) also point to the possibility of path dependence in learning to learn. Firms that have learnt to learn will continue to do so at an increasing rate, while those that have never invested in learning from different experiences will not find it optimal to do so. In the context of alliances, this would imply that heterogeneity in alliance capabilities will persist over time.

There are other reasons to expect firms to learn how to manage and learn from alliances. For example, Hamel (1991) points out that the *perception* of one's learning capabilities can affect interaction with the alliance partner as well. More generally, the idea that some firms have learned to manage alliances does not appear to be in doubt among practitioners as well. Trade publications are replete with the clarion call by alliance experts for the increasing formalization of processes by which a firm can systematize the acquisition or development of an 'alliance capability.'[9] Commonly mentioned components of such a process include having formal systems in place to capture the experience from each alliance, having a central administrative entity to coordinate multiple alliances in which the firm is engaged, and maintaining corporate data bases and newsletters on alliances activity.[10] Relatedly, Mody (1993) explicitly argues that because of the uncertainty inherent in alliances, the design of such organizations may intentionally value flexibility, to the extent that this allows for greater learning and may result in firms acquiring greater competence in managing alliances.

Despite the theoretical support for the idea that

[8]Indeed, Kale and Singh (1999) argue that differences in the organizational processes used to accumulate, codify and share knowledge explain differences in firms' abilities to learn from alliances.

[9]See, for example, Harbison and Pekar (1997) and various issues of the *Alliance Analyst* which are devoted to various aspects of alliance capabilities.

[10]The trade press also implicitly references a life cycle model by which firms acquire alliance capabilities: for example, it is postulated that firms move from managing one-off alliances to a lone-ranger model where alliance capability resides in a small number of individuals, to a more formal model (*Alliance Analyst*, June 9, 1997).

learning to manage alliances might be important, and the widespread practitioner recognition of the importance of learning in alliances, empirical analyses have not focused on this issue, except tangentially. Kogut (1989) identifies the difficulty of managing alliances and, implicitly, the need to develop the ability to manage them. The only related empirical papers show that pairs of firms appear to learn over time to manage their collaborative activities more efficiently (Gulati, 1995),[11] and contracts between firms that have had prior contractual relationships appear systematically different from *de novo* pairings (Anand and Khanna, 2000).

When is learning important in alliances?

We elaborate here on the logic that the importance of learning increases with the difficulty in specifying the process or knowledge in question. Just as learning to manage acquisitions can be expected to be quite different from learning to manage alliances—in that a capability to do the one does not imply a capability to do the other—so also the term 'alliances' encompasses a medley of often vastly different organizational forms. Consequently, a natural question to ask is: when is learning likely to be important in alliances? In this subsection, we distinguish, first, between learning to manage joint ventures vs. learning to manage licenses (the two most common forms of interfirm agreements), and, second, between learning to manage different forms of joint ventures.

It is well acknowledged that the underlying complexity of context will influence the structure of alliances. For example, when knowledge is easy to protect, knowledge transfers are less likely to be susceptible to appropriability and hold-up conflicts between the partners. In such situations, licensing contracts are likely to be the alliance of choice in comparison to joint ventures since they are much more clearly articulated contracts.[12] There are relatively precise criteria available to guide licensing contracts along few, well-specified dimensions. For example, manuals on the appropriate structure of licensing contracts often have reasonably clear formulae for the calculation of royalty payments, exclusivity clauses, territorial restrictions, and other parameters (see Caves, Crookell, and Killing 1983; Parr and Sullivan, 1988). On the other hand, joint ventures are more likely to be observed in situations where alliance partners are faced with greater ambiguity. Indeed, prescriptions regarding joint ventures are confined to advise firms to align interests through equity sharing precisely because rules of 'good management' in these contexts are hard to articulate.

Since, as argued earlier, the potential for firm learning will depend on the extent of ambiguity or complexity of contingencies facing alliance partners, it immediately follows from the discussion above that the extent of learning is likely to be correlated with the structure of alliances. Specifically,

> *Hypothesis 1. Learning effects should be stronger in joint ventures than for licensing contracts.*

A similar line of reasoning suggests that there is likely to be considerable heterogeneity in learning effects within joint ventures. In particular, Pisano's work suggests that ambiguity and uncertainty are greatest in high-technology situations;[13] by implication, learning to manage alliances ought to be most important in R&D situations relative to downstream alliances, suggesting our second hypothesis:

> *Hypothesis 2. Learning effects should be stronger in R&D joint ventures than in other categories of joint ventures (production joint ventures and marketing joint ventures).*

Our empirical implementation below has three main components. First, we develop a measure of alliance-specific experience by firms, using

[11] The evidence here is not about direct value creation through repeated activity, but is an inference drawn from the greater propensity of firms that have allied in the past to do so again.

[12] Prior work has provided empirical evidence that there is a clear relationship between the extent of ambiguity in codifying knowledge and the choice of contract in interfirm alliances: Anand and Khanna (2000) demonstrate empirically that licenses are significantly more frequently employed than joint ventures in contexts where it is relatively easy to establish property rights over knowledge and where ambiguity is low. The reasons that it is easier to specify and communicate technological know-how in some industries than in others have been discussed extensively in various papers, for example Landau and Rosenberg (1992), Arora and Gambardella (1996), Levin *et al.* (1987), and Dam (1995).

[13] See also Harrigan (1988) and Mody (1993).

both publicly available and proprietary data on firms' alliance histories. Second, we use standard event study methodology to create a measure of value creation for alliances. These have been used fairly extensively to study joint ventures, though only very rarely to study other forms of interfirm contractual agreements. None of these prior studies has been concerned with learning effects and the associated development of an alliance capability. Third, a careful treatment of learning effects would need to distinguish these from the role of interfirm unobserved heterogeneity in value creation. The reason is that if learning effects are important, then differences in the age of firms would result in different stocks of experience and consequently differences in value creation. The use of multiple observations on each firm allows us to distinguish between these two effects. Intrafirm temporal variation in value creation allows us to capture the effects of learning, and interfirm (cross-sectional) variation captures the effects of both differences in experience and intrinsic ability.[14] Thus, the panel nature of the data allows us to effectively resolve this identification problem.

DATA AND METHODOLOGY

Data

The data on alliances entered into by firms are drawn from the Strategic Alliance data base of the Securities Data Company (SDC). Such deals include agreements or contracts entered into at various stages of the value chain. SDC obtains information from publicly available sources, including SEC filings, trade publications and international counterparts, and news and wire sources. Although the data base goes back to 1986, SDC appears to have initiated systematic data collection procedures for tracking such deals only around 1989; hence, the deal sample prior to 1990 is far from comprehensive. Even over the 1990–93 sample period, the data clearly would not track all deals entered into by U.S. firms, owing to inadequate corporate reporting requirements. However, since this data base is among the most comprehensive sources of information on such deals, it is a sensible starting point for empirical analysis.

We start with a list of all alliances entered into in the manufacturing sector (i.e., Standard Industrial Classification (SIC) codes 20 through 39) between 1990 and 1993 inclusive, the data extract available to us. This yields 9000 alliances over our sample period in the manufacturing sector. Of these, 71 percent involved at least one foreign firm. We restrict the analysis to those agreements involving at least one U.S. participant (this does not need deals in which there is a foreign participant), as this facilitates obtaining stock price data from common data sources. For the same reason, deals in which all the firms are privately held are excluded from the analysis, since it is difficult to derive value creation measures through alliances for these firms. Since we use contract-specific measures of experience, on the conjecture that managing one kind of organizational form (say, joint ventures) is quite different from managing another kind (say, licenses), we focus on those alliances whose contractual forms are most clearly defined in the data—the sample of joint ventures and licenses. These sample attrition criteria leave us with 870 joint ventures and 1106 licenses.

SDC provides information on various contract-specific characteristics, including contract type (i.e., whether it is a joint venture agreement, licensing agreement, etc.), the identities of the participating firms, the date of the agreement, and the SIC code of the alliance. The SIC code of the alliance may be different from the SIC codes of the participating firms; for instance, a firm whose primary activities are in a particular industry may enter into an alliance in another industry. In order to 'clean' the SDC data, we carried out three major tasks:

Accuracy of data on contract type

We were able to find information about the contractual type of the alliance from non-SDC sources on about 80 per cent of the deals. From our reading of the descriptions of the agreements in Lexis-Nexis, the SDC data on contract type is quite accurate. In some cases, however, alliances

[14] Interestingly, if firm fixed effects are larger for older firms, then we cannot disentangle the effects of past experience from the effects of ability in explaining the unobserved heterogeneity. If the reverse were the case, however, then differences in underlying ability must be large enough to offset the advantage of experience.

are classified in a unique category when in fact the underlying deal appears to be more complex and encompasses more than one type of contract. For example, the transfer or exchange of technology in a licensing deal was, in a few cases, also accompanied by the setting up of a joint venture for purposes of research or marketing. However, such cases are not observed frequently in the data.

For joint ventures, SDC provides additional information on whether these are entered into at the R&D stage or marketing stage. We classify these as research joint ventures, and marketing joint ventures, respectively. The remaining joint ventures mostly involve cooperation exclusively in manufacturing, hence we classify these as production joint ventures.

Accuracy of data on industry of activity

We supplemented the data set with information on various deal-specific characteristics that we obtained from the Nexis-Lexis data base. For some characteristics, such as the information on alliance SIC codes, SDC's information is very accurate. The description of each agreement in the Nexis-Lexis data base is almost always consistent with the 2-digit SIC code within which the agreement is classified by SDC. We do not have a systematic way of checking the accuracy of the 3-digit classification assigned by SDC to a particular agreement. However, even for these, the classifications assigned by SDC appear to be accurate in those cases where we are able to clearly identify the primary area of activity of the alliance.

We categorize industries according to those in which there is significant alliance activity, leaving deals in a miscellany of industries in the 'Other' category. Each separately identified industry in the table corresponds to a 2-digit or 3-digit SIC category selected to account for those categories within which there is significant joint venture or licensing activity. The categories we identify are: Drugs (SIC 283), Chemicals (SIC 28, excluding SIC 283), Computers (SIC 357), Communications (SIC 366), Chips (SIC 367), Cars (SIC 371), and Instruments (SIC 38). Industries of especially high joint venture activity are those labeled 'Chemicals,' 'Chips,' and 'Communications,' while 'Drugs' and 'Chips' account for especially high levels of licensing activity.

Accuracy of data on alliance dates

SDC data on the date of the event are misstated in many instances. For each deal, we attempt to track all mentions of the deal in various news sources, including news and wire reports, newspapers, magazines, and trade publications, listed here in decreasing order of accuracy about the actual date on which the deal was signed. For example, news and wire reports consistently provide information on a particular deal a day or two in advance of newspapers, which in turn are a few days ahead of magazines, and so on. Being able to accurately pin down the date on which the deal was consummated is extremely important for our stock price-based analysis of value creation. Consequently, we spent a major portion of time doing so. In most cases, the extent of inaccuracy of SDC information is within one or two months, and in the majority of cases, within a few days. In some cases, the SDC-reported dates appear to coincide with the date on which the agreement was finally signed; in other cases, the SDC-reported dates seem to coincide with the date on which agreement negotiations appear to have begun. As such, the date information that we end up using is substantially different from that provided by SDC, and in most cases is based on verification across multiple sources.

We obtained firm-specific information from the Center for Research in Security Prices (CRSP) data base as well as the Compustat data base. Such information is available only for publicly listed firms. For such firms, we obtained data on stock price movements for that firm over a 290-day period (−250 through +40) surrounding the data of the alliance announcement. We use these data in arriving at an estimate of the amount of value created in the alliance for each publicly traded participant, based on the methodology described below. For all listed firms, we also extracted various balance sheet and income statement data from the Compustat data base as well.

Methodology

To estimate the incremental amount of value creation for each firm in the alliance, we extract the residuals from a standard asset pricing model used to predict firms' returns. We use daily data on the stock market returns of each publicly listed firm in the data base over a 240-day period prior to the event day (see Brown and Warner, 1985) to estimate the following market model (see Fama, 1976):

$$r_{it} = \alpha_i + \beta_i r_{mt} + \epsilon_{it}$$

Here, r_{it} denotes the daily returns for firm i on day t, r_{mt} denotes the corresponding daily returns on the value-weighted S&P 500, α_i and β_i are firm-specific parameters, and ϵ_{it} is distributed i.i.d. normal. The estimates obtained from this model are then used to predict the daily returns for each firm i over a 14-day period surrounding the event day (i.e., event days −10 through +3), as:

$$\hat{r}_{it} = \hat{\alpha}_i + \hat{\beta}_r r_{mt}$$

where $\hat{r}_{it}$ are the predicted daily returns, and $\hat{\alpha}_i$, $\hat{\beta}_i$ are the model estimates. Thus, the daily firm-specific excess returns can be calculated as

$$\hat{\epsilon}_{it} = r_{it} - \hat{r}_{it}$$

where $\hat{\epsilon}_{it}$ are the daily firm-specific excess returns.

The excess returns thus reflect the daily unanticipated movements in the stock price for each firm over the event period. Together with data on the existing value of a firm's equity, these can be used also in calculating the total value accruing to the firm from the alliance. Of course, *ex post* performance will not be perfectly predicted by these *ex ante* estimates. Instead, these excess returns reflect the expected value that the market believes the firm will capture by entering into the particular alliance. These excess returns may also be thought of as a measure of the 'surprise' element associated with the signing of a contract or alliance. Consequently, to the extent that information regarding particular alliances may leak out prior to the actual announcement of the agreement, the estimate of returns from the alliance will be understated by simply focusing on a 10-day event window prior to the announcement.

The daily firm-specific excess returns can be used also to calculate the daily cross-sectional mean excess returns, μ, associated with the alliance announcements. The test statistic used in evaluating the statistical significance of these cross-sectional mean excess returns is computed as μ_t/σ, where

$$\sigma = \frac{\sum_i \sum_{t=-250}^{-11} (\hat{\epsilon}_{it} - \overline{\epsilon_{it}})}{IT - 1}$$

where $\overline{\epsilon_{it}}$ denotes the mean excess returns (calculated over all firms i over the estimation period). The test statistic μ_t/σ will be distributed unit normal under the null hypothesis for large I, if the excess returns are i.i.d. with unit variance.

RESULTS

Summary statistics

Table 1 presents our measures of experience in managing alliances. $CumJv_{it}$ measures the number of joint ventures entered into by the firm prior to and including the joint venture in question, within the time window of our data. $CumLic_{it}$ measures the number of licensing agreements entered into by the firm prior to and including the current licensing contract. The experience measures thus have a lower bound of 1. There is considerable variation in the experience measure across firms, and for the same firm over time—$CumJv_{it}$ varies from 1 through 23 deals, and licensing experience ($CumLic_{it}$) varies from 1 through 30 deals. 18.7 percent of joint ventures are entered into by firms that have had a recent history (within our sample window) of more than five joint ventures, while 7.08 percent have had a recent history of at least 10 joint ventures. Similarly, 14.8 percent of our licensing deals are entered into by firms that have had a recent history of more than five licenses, while 5.4 percent have had a recent history of at least 10 licenses. The experience measures are left-censored, since they only account for the deals entered into by the firm since 1990.[15] Alternatively, these measures may be viewed as particularly sensible if one assumes that recent experience is more relevant in learning how to manage alliances than is experience on deals that have been consummated in the more distant past.[16]

Before examining the effects of experience on value creation, we first summarize the basic

[15]This introduces measurement error in these variables, which we discuss, later in our robustness checks.

[16]Benkard (1998) provides empirical evidence in support of 'forgetfulness' by firms in the aircraft industry.

Table 1. Distribution of experience measures

Distribution of firm-specific experience measures for a sample of joint ventures and licenses obtained from an extensively cleaned version of the Strategic Alliances data base of the Securities Data Company, 1990–93. CUMJV is the number of past joint ventures entered into by the firm prior to and including the joint venture in question within the time window of our data. CUMLIC is defined similarly for licensing contracts

Joint ventures			Licenses		
CUMJV	Frequency	Percent	CUMLIC	Frequency	Percent
1	384	44.14	1	505	45.66
2	147	16.90	2	193	17.45
3	78	8.97	3	114	10.31
4	57	6.55	4	73	6.60
5	41	4.71	5	57	5.15
6	36	4.14	6	41	3.71
7	29	3.33	7	29	2.62
8	20	2.30	8	19	1.72
9	16	1.84	9	15	1.36
10	12	1.38	10	11	0.99
11	9	1.03	11	9	0.81
12	8	0.92	12	7	0.63
13	7	0.80	13	6	0.54
14	5	0.57	14	6	0.54
15	5	0.57	15	2	0.18
16	4	0.46	16	2	0.18
17	3	0.34	17	2	0.18
18	3	0.34	18	2	0.18
19	2	0.23	19	2	0.18
20	1	0.11	20	1	0.09
21	1	0.11	21	1	0.09
22	1	0.11	22	1	0.09
23	1	0.11	23	1	0.09
			24	1	0.09
			25	1	0.09
			26	1	0.09
			27	1	0.09
			28	1	0.09
			29	1	0.09
			30	1	0.09
Total	870	100.00	Total	1106	100.00

results of the event study analysis in Table 2, panel A. The announcement day is defined as day zero; the table reports the results for both the joint ventures and licensing subsamples, and reports both daily and cumulative excess returns over the event window. The daily average abnormal return on the announcement day is 0.67 percent (z-statistic = 6.93) for joint venture announcements, and 1.42 percent (z-statistic = 11.31) for licensing contracts; both these results are statistically significant at the 5 percent level. The cumulative abnormal returns over the event window are correspondingly large as well: 1.61 percent through day+3 for joint ventures, and 3.13 percent for licensings.[17] Thus, alliances appear to create significant value for the firms involved. The day zero average abnormal returns are similar in magnitude to those observed by McConnell and Nantel (1985) for joint venture

[17]The event day responses suggest that information leakage is not a serious problem in either of our subsamples, since average abnormal returns are not significant for almost all days prior to the event day. For licensing contracts, the preannouncement day and postannouncement day abnormal returns appear to be large, however (0.36% and 0.61%, respectively).

Table 2. Panel A: Event study results

Average daily excess returns and cumulative excess returns for the sample of 870 joint venture announcements and 1106 licensing announcements for firms traded on the NYSE, AMEX, or NASDAQ with available CRSP returns data during 1990–93. Excess returns are the residuals from a market model (Fama, 1976); Brown and Warner, 1985) used to predict firm returns. The announcement day is defined as day 0.

Event day	Joint venture events		Licensing events	
	Daily excess returns	Cumulative excess returns	Daily excess returns	Cumulative excess returns
–10	0.12274	0.12274	0.08478	0.08478
–9	0.00166	0.12441	0.19560	0.28038
–8	0.01091	0.13532	0.02725	0.30763
–7	0.13471	0.27002	–0.00566	0.30197
–6	–0.05906	0.21096	0.10443	0.40640
–5	0.11033	0.32129	0.04661	0.45300
–4	0.0105	0.33178	0.19458	0.64758*
–3	0.13761	0.46940	0.02756	0.67514*
–2	0.25206*	0.72146*	–0.00171	0.67343*
–1	0.05274	0.77420*	0.35857**	1.03201**
0	0.67451**	1.44871**	1.42496**	2.45697**
+1	0.13922	1.58793**	0.60622**	3.06319**
+2	0.04897	1.63690**	0.05976	3.12295**
+3	–0.02671	1.61019**	0.00449	3.12744**
+4	–0.11679	1.49341**	–0.30493**	2.82251**
+5	–0.26608	1.22733**	–0.04667	2.77584**
+6	–0.05074	1.17660**	–0.06612	2.70971**
+7	0.04077	1.21737**	–0.21789	2.49182**
+8	0.17021*	1.38758**	–0.02365	2.46817**
+9	–0.06845	1.31913**	0.27031*	2.73848**

All numbers are in percentages.
*significance at the 10% level; **significance at the 5% level

Table 2. Panel B: Summary statistics from event study

Summary statistics of excess returns and associated wealth effects calculated from the event study for a sample of 870 joint ventures and 1106 licensing contracts. Wealth effects are calculated by multiplying the cumulative excess returns in the event window (day –10, day +1) by the firm's market value of equity 10 trading days before the event announcement date.

	Wealth effects (in $ thousands)					
	Mean	Median	S.D.	Minimum	Maximum	# of observations
Joint Ventures	44.068	0.765	909.141	–11504.080	4198.313	870
Licenses	20.377	1.552	691.048	–6194.596	5549.913	1106

	Abnormal returns					
	Mean	Median	S.D.	Minimum	Maximum	# of observations
Joint Ventures	1.82%	0.72%	10.96%	–40.02%	105.30%	870
Licenses	3.06%	0.82%	15.98%	–42.57%	119.40%	1106

announcements— average abnormal returns over a 2-day window of 0.74 percent compared to 0.81 percent for the corresponding event window in our sample. Similarly, Chan *et al.* (1997) report average announcement day abnormal returns of 0.64 percent for their composite sample of strategic alliances.

To derive the wealth effects associated with these returns figures, we created a measure (labeled 'wealth effect') for each firm that multiplies the 'abnormal returns' measure (defined as the cumulative excess returns in the event window day −10 through day +1) by the market value of equity of the firm on day −10 (i.e., the first day of the event window).[18] Summary statistics for wealth effects and abnormal returns are reported in Table 2, panel B. The mean dollar value created in joint ventures is $44.07 million, with the median value being $0.765 million. In comparison, although the percentage excess returns created in licensing deals is larger (3.06% compared with 1.82% for joint ventures), the value created in these deals is smaller (mean value of $20.38 million, median of $1.55 million), since the participating firms are on average smaller as well. Since the abnormal returns measures reflect the effects of firm size as well, the wealth effect measures may be viewed as a more attractive metric by which to examine the value creation in alliances.[19] Throughout, however, we report the results for both types of measures.

Table 3 presents the variation in performance by industry. For joint ventures, a one-way analysis of variance reveals that neither the mean returns nor the mean wealth effect differ significantly across industry categories (p-value $= 0.24$ for ANOVA of returns, and 0.42 for ANOVA of wealth effect measure). For licensing deals, however, there do appear to be systematic differences in performance across industries. The performance-based rank ordering of industries differs substantially according to the performance measure being used, because of the different size distribution of firms across industries.

Results for joint ventures

Table 4 summarizes the variation in joint venture performance across levels of experience. We categorize the firms' experience measure into four groups: those deals in which the firm has no prior experience, ($CumJv_{it} = 1$), those deals in which the firm is known to have entered into exactly one prior joint venture ($CumJv_{it} = 2$), two or three prior joint ventures ($CumJv_{it} = 2$ or 3), and at least four prior joint ventures ($CumJv_{it} \geq 4$). Table 4 shows that the dollar value created is significantly higher for firms with at least four prior joint ventures, relative to deals in which the firms have been involved in fewer deals. A one-way analysis of variance indicates that the differences in mean wealth effects across the four categories is statistically significant (p-value $= 0.03$). A univariate analysis of the effects of experience on percentage abnormal returns appears to reflect the opposite pattern; however, since returns vary inversely with firm size, the simple cross-tab analysis would be confounded by the effects of size. (Again, the differences are statistically significant across the various experience categories; p-value $= 0.02$).

Regression analyses confirm these findings.[20] In a multivariate analysis that controls for the effects of firm size and industry effects (Table 5, specification (i)), the effect of experience on wealth creation through joint ventures is positive and statistically significant at the 1 percent level. Each additional deal of experience translates into an incremental $42.28 million of value for the firm. Given that the mean value accruing to a firm in a joint venture is $44.07 million, these effects are economically large as well.

When percentage returns are used as the performance measure, the effect of experience is not

[18]This is an approximation to the 'actual' wealth effect which is based on the product of abnormal returns over the event window and given by: $V_{i,day\_0} * \left(\prod_{t=-10}^{t=+1} (1 + \hat{\epsilon}it) - 1\right)$. For our sample, $\hat{\epsilon}_{it}$ is on average 0.1 percent, hence the approximation is very good: for licensing contracts, for example, the correlation between the actual and approximated wealth effects is 0.98; moreover, the distribution of a test statistic based on the sum of returns is straightforward to obtain.

[19]However, since the value creation analysis is based on publicly traded firms, which are likely to be larger than the population average, the wealth effect figures will overstate the average value created from alliances by *all* firms.

[20]Using OLS procedures in the cross-section estimation implicitly assumes that the error term is uncorrelated across the firms *within* an alliance. Ignoring these within-alliance correlations may lead to spurious claims of significance (Moulton, 1986). Therefore, in all the estimations, we relax the assumption of independent errors across firms within the same alliance, while continuing to maintain the assumption of independence across alliances.

Table 3. Wealth effects and abnormal returns by industry category

Industry distribution of frequency of events, and of wealth effects and excess returns from the event study for the sample of 870 joint ventures and 974 licensing deals for which data on industry location of alliance activity are available. Industry categories indicate location of activity of the alliance and are grouped into major categories as follows: Drugs, SIC 283; Chemicals, SIC 28 (excluding SIC 283); Computer, SIC 357; Communication, SIC 366; Chips, SIC 367; Cars, SIC 37; Instruments, SIC 38.

	Joint ventures					Licenses					
	#	Wealth effects (in $ thousands)		Excess returns (%)		#	Wealth effects (in $ thousands)		#	Excess returns (%)	
Industry		Mean	S.D.	Mean	S.D.		Mean	S.D.		Mean	S.D.
Drugs	55	−55.007	901.346	3.25%	11.33%	329	13.232	426.776	386	3.39%	16.12%
Chemicals	133	148.274	812.237	3.27%	11.28%	74	41.721	468.244	83	2.55%	12.39%
Computer	57	−119.557	1732.238	1.40%	13.46%	88	190.607	985.711	106	3.80%	16.51%
Communication	68	225.443	1098.953	3.08%	8.02%	70	−107.584	868.004	81	7.28%	26.01%
Chips	69	94.587	572.908	0.99%	12.13%	192	2.845	871.717	204	2.46%	13.97%
Cars	54	−27.480	655.220	2.59%	9.76%	–	–	–	–	–	–
Instruments	56	−109.668	953.748	0.46%	9.88%	–	–	–	–	–	–
Other	378	37.638	790.716	1.18%	10.92%	221	11.845	685.657	246	1.49%	13.56%
Total	870	44.068	909.141	1.82%	10.96%	974	20.377	691.048	1106	3.06%	15.98%

Table 4. Wealth effects and abnormal returns by experience categories for joint ventures

Variation in wealth effects and excess returns by experience category for the sample of 870 joint ventures. The experience categories are defined as follows. Category 1 indicates deals in which the firm has no prior experience with the joint ventures; category 2 refers to deals in which the firm is known to have entered into exactly 1 prior joint venture; category 3 refers to deals in which the firm has entered into 2 or 3 prior joint ventures; category 4 refers to deals in which the firm has entered into at least 4 prior joint ventures.

Experience category	Joint ventures				
	#	Wealth effects (in $ thousands)		Excess Returns (%)	
		Mean	S.D.	Mean	S.D.
1	384	−0.806	394.042	3.09%	14.06%
2	147	−21.296	637.837	1.35%	9.53%
3	135	−4.055	875.077	0.22%	7.49%
4	204	207.482	1553.038	0.84%	5.75%
Total	870	44.068	909.141	1.82%	10.96%

significant at the 10 percent level, similar to the univariate analysis. One important assumption that is implicit in the analysis thus far is that, controlling for experience, firms have similar capabilities in managing joint ventures. If this were not true, then our estimates of the effects of experience will be biased to the extent that there is any systematic relation between joint venture capabilities and joint venture activity by firms. For example, if the most active firms were also the 'high-quality' firms or the most capable firms in managing joint ventures, then our estimates of the effects of experience would overstate the true effect. The reason is that the superior performance of firms with substantial joint venture experience may be capturing the unobserved superior capabilities of these firms in managing joint ventures. Conversely, if the most active firms were the 'low-quality' firms,[21] then the estimates of the experience effect will be downward-biased. Given that firms have very different approaches and systems in place to manage joint ventures, and probably differ substantially in average quality as well, it may be important to

[21]This is consistent, for example, with a theory in which there is adverse selection of firms entering into joint ventures.

Table 5. Cross-sectional analysis of value creation in joint ventures

Multivariate regression analyses examining the determinants of wealth effects and excess returns for joint ventures. Dependent variable is obtained from the event study on 870 joint ventures from an extensively cleaned version of the Securities Data Corporation Strategic Alliance data base, 1990–93. Experience (CUMJV) codes the number of prior joint ventures entered into by the firm including the deal in question. The assets variable is obtained from the Compustat data base and is measured in millions of dollars. Industry categories refer to the industry location of the alliance activity and are as defined in Table 3. Estimates of firm fixed effects are suppressed. The estimations allow for correlated errors across the firms within the same alliance, while maintaining the assumption of independence between alliances. Heteroskedastic-consistent White standard errors are used in deriving the *t*-statistics, which are reported in parentheses.

Dependent variable	Specification (i) Wealth effects	Specification (ii) Abnormal returns	Specification (iii) Wealth effects	Specification (iv) Abnormal returns
Constant	–104.7340**	0.0167**	–199.3768**	0.0053
	(–2.451)	(2.284)	(–2.528)	(0.606)
Experience	42.2808***	–0.0009	56.7769***	0.0024**
(CUMJV)	(3.760)	(–1.160)	(3.361)	(2.292)
Assets	–0.0003	–1.06e–07*	0.0006	–1.60e–07
	(–0.212)	(–1.663)	(0.150)	(–0.649)
Industry fixed effects				
Drugs	–20.3226	0.0177	130.7760	0.0288
	(–0.140)	(1.021)	(0.997)	(1.213)
Chemicals	129.4546*	0.0203*	219.0927**	0.0312
	(1.672)	(1.777)	(2.212)	(1.782)
Computer	101.1141	0.0012	253.6864	–0.1059
	(0.851)	(0.064)	(1.381)	(–0.427)
Communications	178.7035	0.01982*	467.9587**	0.0283
	(1.402)	(1.589)	(2.447)	(1.663)
Chips	17.5895	–0.0019	126.2548	0.0099
	(0.228)	(–0.122)	(1.030)	(0.576)
Cars	–77.3045	0.0136	83.9343	0.0102
	(–0.808)	(0.935)	(0.975)	(0.626)
Instruments	–148.8016	–0.0073	–150.5685	–0.0273
	(–1.099)	(–0.513)	(–0.802)	(–1.423)
Firm fixed effects			147 fixed effects Included	147 fixed effects Included
Number of observations	869	870	862	863
R^2	0.0412	0.0109	0.1890	0.2649
F-value	2.91***	1.73**	11.94***	66.36***

***Significant at the 1% level; **significant at the 5% level; *Significant at the 10% level

explicitly allow for interfirm unobserved heterogeneity in capabilities. Indeed, a quick analysis of the average returns to the most active firms reveals substantial variation in performance, ranging from –5.3 percent average returns (Caterpillar, Inc.) to +3.6 percent (Pepsico, Inc.).

Specifications (iii) and (iv) therefore estimate 147 firm fixed effects in addition to the variables mentioned earlier, thus controlling for unobserved heterogeneity. Identifying the effects of learning from unobserved heterogeneity is straightforward: the firm fixed effects are identified by differences in average performance across firms that have engaged in more than one deal, whereas the experience effect is identified by variation across deals for the same firm over time. The effects of experience are now observed to be larger than the earlier estimates, and statistically significant. Specification (iii) reveals that each additional deal of experience translates into an incremental $56.78 million of value for the partnering firm (significant at the 1% level). The effects of experience are positive and statistically significant at the 5 percent level even when abnormal percentage returns are used as the performance measure. Each additional deal of experience translates into an incremental 0.2 percent in the event reponse, in a sample where the mean event response is 1.8 percent. Moreover, the joint hypothesis that the firm fixed effects are zero is easily rejected at the 1 percent level. Indeed, more than 15 percent of the variance in performance across deals is accounted for by interfirm differences in fixed effects.

The evidence thus indicates that, first, there are large difference in the unobserved capabilities of firms in managing joint ventures. Second, after controlling for these differences, experience in managing joint ventures appears to significantly increase the returns that firms capture from joint ventures as well. In the remainder of the analysis, we examine the robustness of these results as well as the variation in the effect of experience across contract types.

The identification logic above emphasizes that to be able to distinguish the effect of experience from the effects attributable to capability differences across firms, it is necessary to have multiple observations on a single firm. Consequently, we restricted the data to include only those firms which have entered into at least five joint ventures over the sample period (thus, for which we have at least five data points each), since the identification is clearest for this data sample. Indeed, the effect of experience on value creation is found to be positive and statistically significant at the 1 percent level for both performance measures, with the point estimates being very similar to those obtained earlier.

Next, we examine how the effect of experience varies according to the kind of joint venture being entered into, in particular distinguishing R&D joint ventures, production joint ventures, and marketing joint ventures. $CumRjv_{it}$ is defined to be the number of research joint ventures entered into by firm i prior to time t; similarly, $CumRjv_{it}$ and $CumMjv_{it}$ measure the experience variables for production and marketing joint ventures respectively. The estimation results indicate that the effects of experience on performance are largest for research joint ventures (Table 6, columns (i)–(ii)): each additional deal of experience translates into an additional \$291.8 million of value for the firm in question (statistically significant at the 1% level), or an additional 1.33 percent in abnormal percentage returns (z-value = 1.77). Both these effects are statistically significant at the 5 percent level. For production joint ventures, each additional deal of experience translates into an additional \$87.9 million in value creation for the firm (statistically significant at the 1% level); the effect on abnormal percentage returns is 0.27 percent (z-value = 1.15). Finally, experience does not appear to affect the returns to marketing joint ventures. The point estimates of the effect of experience on performance are thus largest for research joint ventures, followed by production joint ventures, then by marketing joint ventures. This result is discussed in more detail below.

Results for licensing contracts

Extending this analysis, we next look at the effect of experience in licensing deals. As before, we define experience measures for licensing contracts: $CumLic_{it} = 1$ for those deals in which the firm has no prior experience in licensings, $CumLic_{it} = 2$ for those deals in which the firm is known to have entered into exactly one prior licensing contract (either as a licensor or a licensee), $CumLic_{it} = 3$ for two or three prior licensings, and $CumLic_{it} = 4$ for at least four prior licensing deals. Table 7 summarizes the variation in the returns to licensing across levels of experience, and indicates that there is no clear pattern to the effect of experience on value creation through licensings. A one-way analysis of variance indicates that the differences in mean value created across the four categories is not statistically significant (p-value = 0.76). The results of multivariate regression analyses, reported in Table 8, confirm that the effects of experience are not statistically significant at the 10 percent level for any performance measure. Thus, there is no evidence that experience in licensings affects the returns to engaging in such deals. However, as for joint ventures, there do appear to be significant differences between firms in their abilities to manage licensing contracts, as evidenced by the significance of the firm fixed effects in these regressions.

Robustness checks

All the results reported above survive multiple robustness checks. First, the results are not sensitive to the particular event window used. For example, using a smaller, 3-day event window (surrounding the event day) to compute our measures of abnor-

Table 6. Cross-sectional analysis of value creation by joint venture type

Multivariate regression analyses examining the determinants of wealth effects and excess returns for specific types of joint ventures. Dependent variable is obtained from the event study on 870 joint ventures from an extensively cleaned version of the Securities Data Corporation (SDC) Strategic Alliance data base, 1990–93. Joint ventures are classified into Research, Production or Marketing joint ventures as per the SDC data base, cross-checked with information obtained from press releases in the Lexis-Nexis data base. Experience codes the number of prior joint ventures of the type in question entered into by the firm (including the deal in question). The experience measures are thus contract-specific. The assets variable is obtained from the Compustat data base and is measured in millions of dollars. Industry categories refer to the industry location of the alliance activity and are as defined in Table 3. Estimates of firm fixed effects are suppressed. The estimations allow for correlated errors across the firms within the same alliance, while maintaining the assumption of independence between alliances. Heteroskedastic-consistent White standard errors are used in deriving the *t*-statistics, which are reported in parentheses.

Dependent variable	Research joint ventures		Production joint ventures		Marketing joint ventures	
	Specification (i) Wealth effects	Specification (ii) Abnormal returns	Specification (i) Wealth effects	Specification (ii) Abnormal returns	Specification (i) Wealth effects	Specification (ii) Abnormal returns
Constant	334.4443	0.1338**	931.3030**	0.3921***	293.2754	–0.1643**
	(1.134)	(2.307)	(2.449)	(5.396)	(0.562)	(–2.456)
Experience	291.8102***	0.0133*	87.9346**	0.0027	44.6232	0.0047
	(2.910)	(1.740)	(2.522)	(1.176)	(0.416)	(0.942)
Assets	0.0003	1.73e–07	–0.0023	–4.11e–07	0.0027	8.34e–08
	(0.017)	(0.196)	(–0.404)	(–1.132)	(0.342)	(0.313)
Industry fixed effects						
Drugs	–32.5088	0.0389	66.6567	0.0602	357.8014	0.0305
	(–0.205)	(0.877)	(0.834)	(1.249)	(1.452)	(0.836)
Chemicals	8.2214	0.0026	179.7320	–0.0083	105.6508	0.0566***
	(0.037)	(0.052)	(1.282)	(–0.525)	(0.490)	(2.840)
Computer	104.3673	–0.0046	15.3828	0.0229	337.8499	0.0074
	(0.582)	(–0.107)	(0.046)	(0.349)	(1.246)	(0.202)
Communications	–157.1399	0.0469	410.4329	0.0205	384.6353	0.0909***
	(–0.793)	(1.171)	(1.241)	(0.699)	(1.574)	(3.731)
Chips	–34.3221	0.0067	9.8061	0.0181	–122.6241	0.0370
	(–0.202)	(0.194)	(0.050)	(0.498)	(–0.462)	(1.102)
Cars	430.1458	0.0745***	–32.4872	–0.0220	141.7749	0.0494
	(1.399)	(2.716)	(–0.253)	(–1.036)	(1.526)	(1.319)
Instruments	–67.9488	–0.0417	–66.0686	–0.0004	–185.6701	–0.0162
	(–0.326)	(–1.025)	(–0.590)	(–0.013)	(–0.904)	(–0.523)
Firm fixed effects	Included	Included	Included	Included	Included	Included
Number of observations	193	195	449	449	296	296
R^2	0.5934	0.5353	0.2515	0.4176	0.4154	0.5578

***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level

mal returns and dollar value created does not change the results. This window yields performance measures that are highly correlated with the measures used in the estimations reported above. There are also no qualitative differences in the results of our multivariate regressions. A corollary of this is that pre-event day information leakage is not driving any differences in our estimates of learning across joint ventures or licenses.

Second, the frequency of joint ventures and of licenses is not substantially different across each of the four years in our sample; nor are the

Table 7. Wealth effects and abnormal returns by experience categories for licensing contracts

Variation in wealth effects and excess returns by experience category for the sample of 974 licenses. The experience categories are defined as follows. Category 1 indicates deals in which the firm has no prior experience with licenses; category 2 refers to deals in which the firm is known to have entered into exactly 1 prior license; category 3 refers to deals in which the firm has entered into 2 or 3 prior licenses; category 4 refers to deals in which the firm has entered into at least 4 prior licenses.

Experience category	Licenses					
	Wealth effects (in \$ thousands)			Excess returns (%)		
	#	Mean	S.D.	#	Mean	S.D.
1	430	17.841	413.243	505	3.98%	18.83%
2	168	46.355	478.931	193	3.90%	14.94%
3	170	51.171	565.512	187	2.78%	13.49%
4	206	–20.939	1206.511	221	0.46%	10.58%
Total	974	20.377	691.048	1106	3.06%	15.98%

summary statistics regarding abnormal returns or dollar value created across these years. Consistent with this, including time dummies in our regressions does not change any results.

Third, changing the functional form of the dependence of our performance measures on firm size does not alter our results. Fourth, we examined in detail the sensitivity of the results to potential outliers: standard checks performed as per the diagnostics indicated by Belsley, Kuh, and Welsch (1980) do not change any of the results.

Fifth, our experience measures are left-censored. We therefore examined the sensitivity of the results to measurement error in the experience measures (in particular, to the *ad hoc* 1990 cut-off, prior to which experience is not measured), by repeating the estimation for subsets of our data. For example, we repeated the analyses for the events falling in the 1991–93 window, with experience measures counting alliance events only if they occurred in this time window. We find that there are no qualitative changes in the point estimates, though standard errors naturally rise as the sample size is progressively reduced.

Finally, we examined the effect of experience on value creation in the alliance, as opposed to at the firm level. To do this, we combined the daily returns of the firms in the same alliance into a value-weighted portfolio (the returns are weighted by the market value of the firm's common stock 11 trading days prior to the initial announcement of the alliance, and the portfolio is then treated as a 'single security' in cross-sectional regressions). In Table 9, we report the results for the '106' such securities involving joint ventures, using both wealth effects and excess returns as dependent variables. The experience measure, defined to be the sum of the experience measures of all the parties involved in the alliance, is significant at the 5 percent level in both specifications. We find that each additional deal of experience in joint ventures translates into an incremental 0.2 percent in the event response, virtually identical to the firm-level results. This suggests that the wealth effect of experience is largely due to value creation as opposed to value division among joint venture partners. A similar exercise for licensings does not reveal any learning effects, as before.

In concluding, it may be worthwhile commenting on a possible alternative interpretation of our results on learning. A positive effect of the number of alliances on the announcement effect may be argued to reflect *market* learning rather than firm learning. For example, market uncertainty about a firm's capabilities may result in a lower market response early on; as this uncertainty declines over time, the announcement effect would increase as well. While plausible in principle, there are various reasons why we believe this may not be a compelling interpretation of our results. First, our data span a relatively short panel, making it less likely that differences in perceptions about firm abilities are likely to be large over this time period. Second,

Table 8. Cross-sectional analysis of value creation in licensings

Multivariate regression analyses examining the determinants of wealth effects and excess returns for licenses. Dependent variable is obtained from the event study on licenses from an extensively cleaned version of the Securities Data Corporation Strategic Alliance data base, 1990–93. Experience (CUMJV) codes the number of prior joint ventures entered into by the firm including the deal in question. The assets variable is obtained from the Compustat data base and is measured in millions of dollars. Industry categories refer to the industry location of the alliance activity and are as defined in Table 3. Estimates of firm fixed effects are suppressed. The estimations allow for correlated errors across the firms within the same alliance, while maintaining the assumption of independence between alliances. Heteroskedastic-consistent White standard errors are used in deriving the *t*-statistics, which are reported in parentheses.

Dependent variable	Specification (i) Wealth effects	Specification (ii) Abnormal returns	Specification (iii) Wealth effects	Specification (iv) Abnormal returns
Constant	–21.0478	0.0280***	–62.0633	0.0358***
	(–0.449)	(2.817)	(–1.011)	(2.681)
Experience (CUMLIC)	–4.3660	–0.0037***	–8.6097	–0.0031
	(–0.476)	(–2.599)	(–0.511)	(–0.731)
Assets	0.0033	–2.48e–07**	0.0126**	–7.93e–07
	(1.290)	(–2.176)	(2.011)	(–1.026)
Industry fixed effects				
Drugs	37.8096	0.0155	–21.3969	0.0065
	(0.746)	(1.189)	(–0.363)	(0.340)
Chemicals	37.6568	0.0056	–8.1033	0.0184
	(0.517)	(0.384)	(–0.073)	(–0.688)
Computer	179.7614	0.0318	327.7553*	0.0398*
	(1.607)	(1.517)	(1.792)	(1.662)
Communication	–99.3360	0.0693**	–128.3829	0.0597**
	(–0.828)	(2.162)	(–0.803)	(2.203)
Chips	7.0889	0.0107	79.3717	0.0220
	(0.087)	(0.782)	(0.523)	(1.070)
Firm Fixed Effects			Included	Included
Number of observations	974	974	974	970
R^2	0.0196	0.0215	0.1336	0.2914
F-value	0.88	3.39***	32.97***	2.10***

***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level

the learning effects that we obtain are robust to the inclusion of time dummies, as mentioned above. Third, the pattern of cross-alliance differences in learning effects is more difficult to explain based on this interpretation, since this would require market perceptions to be different for joint ventures and licensings, and between various kinds of joint ventures as well, in the particular direction that is consistent with the results.

Examining firm fixed effects

Each firm fixed effect can be interpreted as a measure of that firm's alliance capability. It represents the market's perception of the amount of value that will be created, or destroyed (for those firms whose estimated capabilities are negative), on average, when that firm engages in an alliance. We examine here the distribution of estimated joint venturing capabilities and of licensing capabilities for a select sample of the firms. Table 10 reports summary statistics for these fixed effects for those firms for which we have four or more alliances in our sample (41 firms for our joint venture subsample, and 57 for our licensing subsample). Separate firm fixed effects are obtained from the estimations using dollar value and abnormal returns measures, for both the joint ventures and licensing subsamples (this accounts for the four columns in the table).

Table 9. Cross-sectional analysis of value creation in joint ventures' value-weighted portfolios

The value creation measures (wealth effects and excess returns) are obtained by combining the daily returns of firms in the same alliance into a value-weighted portfolio, which is then treated as a single security in the cross-sectional regressions. The daily returns of firms are obtained from the event study on the sample of joint ventures from an extensively cleaned version of the Securities Data Corporation Strategic Alliances data base. Total experience is defined as the sum of the experience measures for all the firms involved in the alliance for which data are available. The assets variable is obtained from the Compustat data base and is measured in millions of dollars. Industry categories refer to the industry location of the alliance activity and are as defined in Table 3.

Dependent variable	Specification (i) Wealth effects	Specification (ii) Abnormal returns
Constant	–306.7108	–0.0114
	(–1.564)	(–1.189)
Mean assets	–0.0024	–1.60e–07
	(–0.356)	(–0.881)
Total experience	46.8977**	0.0017**
	(1.949)	(1.973)
Industry fixed effects		
Drugs	676.341	0.0078
	(0.757)	(0.211)
Chemicals	–125.0108	0.0100
	(–0.317)	(0.484)
Computer	–203.3869	0.0196
	(–0.457)	(0.702)
Communication	331.074	0.0035
	(0.723)	(0.265)
Chips	366.5947	0.0167
	(1.210)	(1.521)
Cars	488.7313*	0.0431***
	(1.912)	(3.564)
Instruments	–338.5916	–0.0217
	(–1.063)	(–1.081)
Number of observations	106	106
R^2	0.1072	0.1000
F-value	1.79*	2.76***

***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level

The mean of the distribution of fixed effects is negative in both subsamples, with large standard deviations. Further, the percentage of firms with negative fixed effects is significantly higher for joint ventures than for licenses (65% vs. 46% using the estimated coefficients from the wealth effects regressions; 80% vs. 61% using the estimated coefficients from the abnormal returns regressions). The wide dispersion in estimated alliance capabilities is in accord with practitioner reports of the wide variation in alliance capability. At first, however, the fact that the most active firms appear to have negative fixed effects runs counter to the intuition that these firms—because of their higher stock of experience—will also be better at managing alliances. Of course, there may be many other explanations for these differences in the fixed effect means between active and less active firms. If alliances represent a 'second-best' option for firms looking to commercialize their innovations (relative to internal commercialization, for example), then firms that are most active in alliances are likely to be the ones with poorer technologies as well.[22]

Ultimately, internal firm data are required to parse out the correlates of our estimated fixed effects.[23] However, some analyses based on publicly available data yield the following interesting conclusions. First, certain firms that are celebrated as being excellent alliance managers do in fact have high fixed effects for both licences and joint ventures: Hewlett-Packard and Coca-Cola are leading examples. However, this is not always true: for example, the evidence regarding Corning—a firm with a reputed alliance capability—is rather mixed.[24] The point estimate for this firm lies roughly in the middle of the sample for whom summary statistics are reported in Table 10. In the case of those obtained from the estimations for R&D joint ventures, separate regressions of these fixed effects on average R&D intensity (defined as the average of R&D/sales for the years in the sample, 1990–93) showed no statistically significant relationship. Thus, there does not appear to be any evidence that differences in the rate of expenditure on R&D drive

[22]There may be other self-selection arguments as well. For example, Anand and Galetovic (1998) offer an equilibrium explanation for why the quality of technologies developed by firms with broader research portfolios (hence, by definition, more active in alliances) is likely to be poorer than the quality of single-technology firms.
[23]See, for example, Henderson and Cockburn's (1994) study of firm fixed effects in pharmaceutical R&D.
[24]See, for example, descriptions of the components of Corning's and of Hewlett-Packard's alliance capabilities in the *Alliance Analyst*, February 17, 1997, and of Coca-Cola in the *Alliance Analyst*, December 9, 1996.

Table 10. Summary statistics: Derived fixed effects.

Summary statistics of firm fixed effects obtained from cross-sectional regressions in Table 5 (joint ventures) and Table 8 (licenses) using only those firms which have four or more alliances (of the type in question) in our data

	Joint Ventures		Licenses	
	Dollar value (in $ thousands)	Abnormal returns	Dollar value (in $ thousands)	Abnormal returns
Number of observations	41	41	57	57
Number of observations with positive fixed effects	14	8	31	22
Mean	–99.98	1.70%	–92.77	0.39%
S.D.	355.170	2.68%	352.895	11.06%

differences in our measure of alliance capability. We also found that an approximation for Tobin's q[25] was not significant in explaining either the point estimates obtained from the licensing regressions or the point estimates obtained from the joint venture regressions. To the extent that Tobin's q is a measure of the intangible general management ability of the firm in question, the latter does not appear to translate over into an alliance capability.

DISCUSSION

We summarize our results around two sets of findings concerning: (a) learning effects in value creation through alliances, and the associated phenomenon of (b) heterogeneity in alliance capability. We find strong evidence that firms learn to create more value as they accumulate experience in joint venturing, whereas there is no evidence that firms learn to create value as they accumulate experience in licensing (Hypothesis 1). These learning effects appear to exist especially in R&D and production joint ventures but not in marketing joint ventures (the result on marketing joint ventures is inconsistent with Hypothesis 1). Consistent with Hypothesis 2, learning effects are stronger in R&D joint ventures than they are in other forms of joint ventures. Finally, we find strong and persistent differences across firms in their ability to create value, in all our alliance subsamples; we interpret these as reflecting differences in 'alliance capabilities.'

As far as we are aware, this is one of the first studies to establish systematic evidence for the existence of significant learning effects in the management of alliances. The magnitude of these effects suggests that the valuation of alliances cannot afford to ignore the dynamic, cross-alliance benefits of entering into a particular partnership. In addition, the results on cross-alliance differences in learning effects also suggest limits on the set of contexts in which returns to experience are likely to be significant, thus implicitly answering the question of when learning is likely to be important. Finally, our results on the explanators of firm fixed effects indicate that it may be important to distinguish between a firm's intangible, general-purpose skills (as embodied in a measure of q) and its alliance capability: strength in one arena clearly does not imply a presence of the other.

There are other interesting issues related to the reasons underlying learning effects, however, which we cannot shed light on with our data. For example, we cannot distinguish whether learning occurs by firms getting better at screening their alliance partners, or because they get better at interfacing with these partners (perhaps through designing better contracts or through getting more adept at managing relationships). Similarly, it would be of both positive and normative interest to examine the extent to which learning rates differ across firms, and, if so, what explains these differences. Finally, our results clearly establish the existence of differences in 'alliance capabili-

[25]A proxy for Tobin's q was defined as: (market value of common stock + book value of preferred stock + book value of debt)/(book value of total assets). Similar proxies have been used in various contexts by Lindenberg and Ross (1981) and by Montgomery and Wernerfelt (1988), among others.

ties' across firms, and estimates of these capabilities (via the firm fixed effects) as well. This could serve as a useful platform for further work which explores the organizational determinants of this alliance capability, following analysis of the sort conducted by Henderson and Cockburn (1994). This would require data internal to each firm regarding the organization of their alliance management processes, possibly collected through surveys.

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